STUDYING COURTS IN CONTEXT

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I. INTRODUCTION

Civil courts and litigation, long a neglected area of legal scholarship (Hurst, 1981), have begun to receive more attention and due recognition of their important role in the legal system and society. Analyses of what the civil courts do, and fail to do, are taking their place beside the numerous studies of the criminal courts. A critical literature has emerged, with some arguing that our system provides no effective forum for many civil complaints (Nader, 1980) and others expressing concern that the adversarial mode of dispute processing—the hallmark of Anglo-American civil procedure—may be costly and inappropriate for many conflicts (Fuller, 1978; Horowitz, 1977; Simon, 1978). Critics have also pointed to rising litigation rates and court caseloads, generated spectres of a "litigation explosion" or "crisis in the courts," and worried about excessive litigiousness (Barton, 1975; Manning, 1977; Rosenberg, 1972).

The Civil Litigation Research Project (CLRP) is one effort to increase knowledge about the role of civil courts in the United States and the nature and function of other institutions which deal with the sorts of disputes typically found in our civil courts, as well as factors that influence decision making in litigation. CLRP was set up under a contract between the University of Wisconsin and the United States Department of Justice. The project was developed jointly by scholars at Wisconsin and the University of Southern California, with assistance from other universities and organizations.[1]

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The principal purpose of the project was to design, conduct, and analyze surveys of participants in civil lawsuits and similar controversies. The surveys were designed to provide some information on who used the courts, and who did not; the reasons litigants chose to initiate suits, pursue claims to the end, or settle; and the way lawyers and clients made litigation decisions. Additional data were sought on the availability of alternative fora for the kinds of cases typically handled by civil courts. Interviews were conducted with lawyers and disputants from a five-state sample of persons and organizations involved in civil lawsuits in federal and state courts and in cases filed in arbitration and administrative agencies. Disputants in controversies similar to civil lawsuits, but never brought to court, were also interviewed. A screening survey elicited some information on the disputing experience of households in the sample areas.

A major concern of CLRP has been to secure data on the costs of civil litigation and similar controversies, at least from the viewpoint of the disputants. This interest in "costs" has two dimensions. At the purely descriptive level, we are interested in knowing the amount and nature of expenditures made by individuals and organizations in lawsuits, arbitration proceedings, and other civil dispute processing settings. On a more analytical level, we have sought to explain why parties invested the sums they did in these controversies, and to relate investment to such factors as stakes and outcomes. We secured detailed information on the legal fees and other expenses involved in the cases and disputes studied. Levels of investment will be compared with a variety of factors thought to influence party expenditure.

The survey of participants in civil disputes which formed the basis for CLRP, was designed to collect general data on dispute experience in various fora in order to illuminate the nature, causes, and effects of the costs of litigation. But the project has also had a broader dimension. During the three years of its operation, CLRP has served as stimulus to, and forum for, debate on a wide range of research issues. This wider scope evolved as the result of several factors. In the first place, since the original project was very generally defined, substantial theoretical and methodological work was a prerequisite to designing the survey instruments. Second, we spent some time, in the course of the project, exploring the possibility of additional forms of empirical inquiry on these matters, including a prospective panel study of dispute

experience over time, and a national census of civil disputes. While these additional studies were never funded, the effort to identify and transcend the limits of the basic CLRP design generated valuable ideas.

The papers in this section of the special issue are primarily the result of the more general dimension of the CLRP enterprise. Since the data from our basic survey only became available for detailed analysis in the spring of 1981, we cannot as yet report the core empirical findings of the project. What we are able to do at this time is to make available a selection of the studies conducted and commissioned in the early phases of survey design and in the period in which we envisioned further studies and alternative methods of empirical research.

Some of the papers which follow (Kritzer, Felstiner-Abel-Sarat, Miller-Sarat) were written by core members of our team. Others (Johnson, Gollop-Marquardt, Coates-Penrod) were commissioned by CLRP and prepared by scholars working closely with us. They represent a wide diversity of topics and disciplines. Nevertheless, one theme unifies them. They all reflect aspects of our effort to define an approach to the study of civil litigation which uses the "dispute" as the prism through which to view civil courts and their role in society. In the earliest stages of CLRP, we decided to examine civil courts as processors of disputes and to study litigation in the context of the disputes that led individuals and organizations to initiate lawsuits. At the same time, we sought information on disputes that were not taken to courts—i.e., controversies that, while similar to those found in lawsuits, were processed by other institutions, or not at all. This approach highlights the dispute processing dimension of what civil courts do and focuses attention on all disputes of a similar nature, regardless of processing mode. For this reason, I shall call it the "disputes-focused approach" to civil litigation. The papers in Part Two set forth the reasons CLRP adopted this approach, the way we sought to implement it through survey methods, and what we learned about the potential and the limits of this way of studying civil courts in society.

We claim no originality for the decision to use the dispute as a unit of analysis in a study of the role of civil courts. Anyone familiar with the literature knows that the disputes focus has been used before (e.g., Sarat and Grossman, 1975; Lempert, 1978). But it is for just this reason that we think these papers will prove of general interest. While we did not invent the idea of studying civil litigation by looking at

disputes, we have had substantial time to explore the implications of this strategy. Part Two of this special issue reports on what we have learned so far.

We have divided Part Two into four subsections. The first is this introduction, in which I shall describe in more detail what I mean by a disputes-focused approach, explain why CLRP adopted this perspective, and outline the way we developed it. The next deals with the survey as a method of research on disputes: we explain our overall research strategy, describe how we translated a general orientation into a concrete research strategy and survey design, and report on the methodological lessons we have learned so far (Kritzer). We also report the results of our first survey, which allowed us to produce estimates of the incidence of civil grievances and disputes involving individuals (Miller-Sarat).

The third subsection illustrates some of the conceptual and theoretical work done by CLRP. Prior to, during, and after the design of the surveys, the project staff debated the best ways to obtain data on disputes and explain the behavior we hoped to observe. These debates generated a series of papers clarifying the meaning of "dispute," explaining the sources of disputes, and modeling disputant behavior. Some of these studies were used in the design of the survey and the drafting of questionnaires. For example, early versions of Johnson's essay on the economic incentives of lawyers and the effects of these incentives on litigation decisions helped us in questionnaire construction. Other studies were prepared after we had substantially completed the survey design phase. The paper by Felstiner, Abel, and Sarat, for example, was part of our effort to analyze the limits of the retrospective study of disputant decisions which forms the core of CLRP's empirical work, and design a prospective panel study which was never funded. This and some of the other papers in this part reflect our recognition of the limits of the surveys we had designed and, indeed, of the survey method. They point to the need to expand the scope of inquiry on disputes, develop new methods, and enrich explanatory theory.

The final section we call "Evaluations." We asked three scholars outside CLRP to read the papers we had produced so far and comment on them for this issue (FitzGerald, Lempert, Kidder). Their comments provide valuable reflections on the dispute focus. Finally, in a brief afterword I attempt to set forth some of the issues that have been presented by the discussion of the disputes focus within CLRP and between

CLRP and commentators outside the project. While the material in this part overlaps with the concerns of our basic survey, the reader must bear in mind that most of these papers and evaluations do not deal with the core CLRP data, which will become available over the next few years.[2]

II. THE DISPUTES-FOCUSED APPROACH

A disputes-focused approach seeks to isolate and study a particular social relationship called the "dispute." It is important to recognize that the object of such study is something carved out, indeed constructed, from social reality. Disputes, in this sense, are not concrete entities, like bees in hives. Nor are they formally recognized social relationships like families or unions. The idea of a dispute may correspond with accepted folk categories, but it need not: parties can have a dispute without giving that label to their relationship. Dispute overlaps with but is not co-extensive with civil lawsuit. Not all lawsuits are disputes, and few disputes become lawsuits. The concept of a dispute is like that of a "social movement"; it stems from a "theory" of society and requires a nominal definition.

Why would one want to study disputes? Given such an interest, how can one go about isolating disputes and securing information about them? What has the study of disputes to do with civil litigation or, indeed, with the study of law in general?

These questions are of importance to the law and society community. CLRP was not alone in developing this approach into a more systematic research strategy. For some years a worldwide "Dispute Treatment" Project has been underway under the auspices of the Institute for Sociology of Law for Europe and the Vienna Documentation Center.[3] Like CLRP, this Project has employed the dispute as a principal unit of analysis.

There is really nothing new about all this. We have always thought of civil courts as institutions designed to settle peaceably some of the conflicts that arise in society. Indeed, it probably came as a surprise to many when Friedman and Percival (1976) announced that courts were playing *less* of a role in dispute processing today than they had in the past. But

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[2] Field work ended in the Fall of 1980. A first report, analyzing some of the data collected, will be submitted to the Department of Justice by the end of 1981.

[3] This project involves a series of teams from various countries who are all studying aspects of the treatment (processing) of disputes in their respective countries. For details, see Blegvad (1979).

what is new is the effort to develop a systematic research strategy for asking questions about this judicial role and exploring it empirically.

Many in the law and society field have been influenced by the disputes focus; some are seeking to develop it and apply it to particular problems. But there is no general agreement on what this approach really means. Nor do scholars agree on whether it is really desirable to try to study courts as dispute processors, compare lawsuits with other disputes, or isolate the dispute from other social relationships. What I have called the disputes-focused approach is little more than a general set of orientations. Even among those who share this approach, there is disagreement on how to apply it to specific issues and tasks.[4] Some are opposed to making the effort at all. They argue that the effort to look at specific disputes and to treat courts as dispute processing institutions is as likely to be misleading as it is to illuminate (Engel, 1980; Tushnet, 1981; Kidder, 1981).

CLRP struggled through several years to translate the disputes focus into an actual research design. We have learned a lot about what can and cannot be done. Our decision to use the dispute as the orienting idea for court research was a response to practical needs and theoretical possibilities. The disputes-focused approach was adopted to do a job, and to exploit what appeared to be exciting possibilities for the development of theory. To explain our choice, therefore, we must trace two aspects of the project's history: its practical goals and our view of the theoretical situation when we started our work.

III. THE PRACTICAL GOALS OF THE PROJECT: NEW APPROACHES TO COURT REFORM

The CLRP surveys were commissioned for use by the Justice Department's Office for Improvement in the Administration of Justice (OIAJ), a small office with a long name set up in 1977 by Attorney General Griffin Bell. Bell, like others at the time concerned with court reform, was convinced of the need to create "alternatives" to litigation in many areas.[5]

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[4] Disagreements can be found in this issue itself. Compare, for example, the definition of "dispute" used by CLRP (Miller and Sarat, 1981: 526) with those proposed by Lempert (1981: 708) and Yngvesson and Mather (1981: 776).

[5] Bell saw the goals of legal reform as: "to assure access to effective justice for all citizens. . . (3) to reduce impediments to justice unnecessarily resulting from separation of powers and federalism, and (4) to increase and improve research in the administration of justice" (1978: 53). For a discussion of the OIAJ program, see Sarat (1981).

Indeed, one of the reasons this special office was set up was to further explore the interest in alternatives. The CLRP surveys were designed, *inter alia*, to provide information useful for appraisal of possible reforms in judicial administration that would divert some civil business from the courts to other institutions.

This background influenced the design of CLRP. Although OIAJ was concerned with many traditional questions, its agenda included an interest in "alternatives" and "access to justice." This meant that issues not usually on the research agenda of court reformers and judicial administrators were included in the planning of CLRP. Most court reform has been concerned with making courts better. Most reforms have been designed to improve the efficiency of existing procedures and facilitate the management of traditional business. Research on courts has largely been oriented toward identifying problems requiring procedural reform and evaluating innovations (Schroeder, 1980).

In the 1970's, however, people started talking about a new kind of reform. Instead of improving judicial administration, we were urged to create alternatives to courts. It was decided that we faced a "crisis" in the courts. Assuredly, this alleged crisis was somewhat paradoxical: it seemed to involve a massive increase in the use of courts, coupled with simultaneous public disillusionment with the judiciary and a recognition that the courts were not handling some business at all. Americans were pictured as rushing to the courts in increasing numbers. They were, at the same time, apparently discovering that there were many things the courts failed to do. And they seemed to express increasing dissatisfaction with the judiciary, at least when asked about such matters by researchers.

To some, this "problem" seemed so big that it required dramatic new solutions (or at least the rediscovery of some old ideas that had been forgotten). People began to argue that we might have to change some of the business of the courts, reducing the need for judicial involvement in some disputes by diversion to alternative institutions. Attention turned to the potential of arbitration and mediation as alternatives to judicial dispute processing (Sander, 1976; Danzig, 1973; Nader and Singer, 1976). New or remodeled institutions, which could handle small complaints on a mass basis, were proposed (Ford Foundation, 1978; Ruhnka and Weller, 1978). Experiments with

diversion schemes like the Neighborhood Justice Center were undertaken (McGillis and Mullen, 1977; Cook *et al.*, 1980).[6]

The debate generated by fears of a litigation explosion and a crisis in the courts seemed to be about two rather different things (Hurst, 1981). First, there was the problem of judicial meddling in things some thought were better left to legislatures and executive agencies. Conservative critics chided the judiciary for going beyond their proper role and capabilities in "extended impact" cases or other public policy controversies (Glazer, 1975). Second, there was the argument that the judicial process and the adversary system seemed inappropriate for a number of conflicts, such as family disputes (Mnookin and Kornhauser, 1979), neighborhood controversies (Felstiner, 1974; Merry, 1979), claims involving small amounts of money ("minor disputes") (Eovaldi and Meyers, 1978; LEAA, 1974; Sarat, 1976), and problems arising in the management of long-term commercial relations (McNeil, 1978).

Most of the interest in alternatives to litigation was related to the second of these issues. Of course, "alternatives" actually covered a very broad spectrum. It encompassed experiments which retained most of the elements of standard judicial approaches as well as efforts at radically restructuring the machinery of dispute resolution. It ranged from minor modifications of the way courts conduct their business to experimentation with new institutions that would employ very different techniques to resolve disputes.

Contrast, for example, the interest in court-annexed arbitration with proposals for wider use of mediation. There have been numerous experiments with mandatory arbitration of cases brought to civil courts (e.g., Johnson *et al.*, 1977). While these plans vary considerably, all retain crucial features of the judicial approach. Disputes enter this system only after they have been defined as legal claims and lawyers have been brought in to represent the parties. Issues of fact and law are defined and argued. Reliance is placed on the adversary process. A neutral arbiter is given the power to make a determination of the issues. Most mediation experiments, on

[6] The Pound Conference held in 1976 to explore "The Causes of Popular Dissatisfaction with the Administration of Justice" signaled a conscious effort to initiate a new era in reform thinking. Setting the tone for the Conference, Chief Justice Warren Burger stressed the need to develop "new machinery for resolving disputes" and for systematic planning for civil justice (Burger, 1976). Other speakers followed the Chief Justice's lead. For example, Professor Frank Sander noted that, "we are increasingly making greater and greater demands on the courts to resolve disputes that used to be handled by other institutions of society." Noting that the courts alone could not respond to such accelerating demands, he concluded that it had become "essential . . . to examine other alternatives" (Sander, 1976: 114).

the other hand, downplay the role of lawyers, make no effort to restate the issues in legal terms, and rely on consensual agreement between disputants (Felstiner and Williams, 1980). Both are alternatives to litigation, but each is of a very different order.

At first blush, it would seem that the interest in alternatives and the conservative critique of an activist judiciary are unrelated. And, to be sure, at a practical level they are: the question of the adequacy of the judicial process to manage and reform, say, the mental hospitals or prisons of a state, has little in common with the issue of how best to handle vast numbers of family or neighborhood controversies, or provide redress for aggrieved consumers. But there are, nevertheless, some ideological affinities between the critique of the judiciary and the interest in alternatives (see Tushnet, 1981). Both lines of thought stressed the incapacity of judges to understand the basic values of parties to a conflict and the likelihood that they would impose unwanted solutions ultimately harmful to social cohesion. Where critics of judicial activism feared that aloof judges would impose unwanted solutions on reluctant communities, proponents of mediation and other alternatives emphasized the virtues of mediated consent over adjudicated and thus imposed right. Indeed, proposals like those for mediation rather than the adjudication of complex public policy issues like environmental controversies brought the two streams of thought together.

Whatever may be the ultimate impulse behind the move towards alternatives, it is easy to see how it created interest in the study of disputes. Proponents of alternatives argued that courts handled some matters badly, that society provided no other machinery for assisting in the resolution of such matters, and that this situation resulted in inefficiency and injustice. To assess these claims, and to develop solutions if they proved valid, it seemed desirable to find a way to identify and describe conflicts which did not reach the courts, as well as to compare the performance of courts with that of other possible arrangements for resolving conflicts and protecting rights. These tasks called for a common unit of analysis, some way to compare the controversies in courts and other institutions; it was also necessary to identify potential judicial "business" that never reached the courts. The answer to these problems was found in the dispute, and the idea of dispute processing.

The dispute was conceived of as the common denominator uniting events outside our institutional machinery with those

handled both by courts and by other forms of third-party dispute processing mechanisms. If similar disputes could be identified in courts and in other settings, and if the impact of different institutions on such disputes and disputants were measured, the dispute focus would answer the need for functional analysis and institutional comparison. In this way, disputes-focused research would provide information on the need for, and viability of, alternatives to litigation, and would also clarify the functions and efficacy of the courts.

CLRP emerged from this background. Although OIAJ was probably primarily concerned with the costs of litigation when it commissioned the surveys, it also sought information on alternative dispute processing institutions and facilities. The CLRP team was able to take this concern with alternatives and shape the project into a comparative study of dispute processing approaches.

IV. DISPUTES AS A LINK BETWEEN LAW AND SOCIETY

The appeal of the disputes approach went beyond its utility in informing policy choice through assessing the performance of courts and alternatives to litigation. It also seemed like the best way to develop an important area of sociolegal studies, building on and significantly expanding an existing research tradition. The disputes-focused approach offered the possibility of greater insight into the social relations and conflicts behind the formal structure of a lawsuit. The dispute was a conceptual link between law and society. It permitted us to see courts in a much broader context. It allowed us to draw on a large corpus of social science learning and pointed toward more powerful theories to explain behavior.

The first stage of our work was an assessment and critique of prior research on courts. This critique led to two conclusions. First, prior studies had not always adequately defined the appropriate domain for research. Researchers on courts had sometimes failed to relate litigation to the social world in which it is embedded; researchers on dispute processing had not yet fully incorporated the courts into an overall concept of disputing and dispute choices. Yet, our purposes demanded that we do both at the same time—that we study, as it were, *courts in context*. Second, our critique suggested that existing theories of dispute processing suffered from an unfortunate disciplinary balkanization. There were studies of dispute behavior by anthropologists, sociologists, psychologists, and economists. Each of these approaches to the

explanation of dispute behavior had something to offer. But a multidisciplinary approach would permit a more comprehensive account of disputing.

The Need for an Expanded Scope of Inquiry

Our review of prior literature suggested the importance of expanding the scope of inquiry. We found numerous studies of courts and some research on "context." But we found few studies which looked at all the factors we thought were relevant to understanding courts *in* context.[7] Little effort had been made to define the universe of events which might end up in court or to examine decisions to use or not use the courts. Nor had students of litigation investigated the full range of variables influencing decisions made by litigants and their attorneys. We did find some research on the outcomes of litigated cases; however, this work was limited in scope. Studies had been conducted of personal injury litigation and auto accident claims. Some effort had been made to observe outcomes and relate them to formal claims or alleged injury; but these studies did not look at the effect of litigation on the underlying relationship between parties, where these existed, and were, in any event, limited to just a few areas of law. In addition, there were numerous studies of court performance and evaluations of procedural reforms. But performance research focused primarily on administrative matters like delay and congestion. Finally, evaluation of reforms was limited to measuring the effect of procedural changes, such as pretrial conferences, on court performance: little effort had been made to compare courts with other dispute processing institutions.

This is not to say that prior studies were without value or that we did not lean heavily on our predecessors in the design of CLRP. Studies of the use and non-use of courts in some areas (e.g., Macaulay, 1963) and of the effects of party configuration on disputant choices and outcomes (e.g., Galanter, 1974) proved valuable, as did numerous studies in the tradition of legal anthropology. We learned a lot about the incidence of various legal problems and propensity to use lawyers from the "legal needs" studies, even though these were concerned primarily with lawyer use rather than with what FitzGerald and Dickins (1981) have aptly called "alternative dispute trajectories." Specific studies of litigation in areas like auto accidents (Conard *et al.*, 1964), personal injury suits

[7] A catalogue of most of the major research on courts was prepared for CLRP. See Schroeder (1980).

(Hunting and Neuwirth, 1962), and insurance claims (Ross, 1970) provided insights regarding patterns of settlement, use of discovery, frequency of trials, and similar essential parameters. What we found missing in the prior work was an approach which could incorporate the major dimensions of experience related to civil legal disputes and integrate information on the incidence of disputes, the behavior of disputants, the choice of alternative trajectories, and the impact of courts and all other institutions which regularly deal with matters similar to those handled in lawsuits. The CLRP surveys were designed as a beginning effort to fill some of these gaps.

The Need for a Multidisciplinary Approach

In addition to expanding the range of behavior to be investigated, we saw a need to enlarge the framework for explaining this behavior. This need grew out of the recognition that our primary task would be to observe and ultimately explain a wide range of *dispute decisions*—whether or not to make a claim, use a lawyer, seek assistance from some third party (court, other institution), settle, demand a hearing, etc. Available research on the dynamics of the dispute decision was limited in scope and suffered from the "disciplinary balkanization" mentioned earlier, in which scholars working in various social science fields had given some attention to the factors underlying dispute decisions, but had tended to focus on only one aspect of what we perceived to be a unitary experience. We felt that it might be possible to develop a more comprehensive approach to the explanation of dispute decisions by combining the insights of these fields.

The Disputes Focus as a Way to Transcend Limits of Prior Research

The disputes-focused approach thus offered a way simultaneously to expand the scope of inquiry and enrich theories of dispute behavior. Isolating disputes wherever they occurred or were processed would enable us to integrate information about lawsuits with data from other dispute processing institutions and from controversies in which no third party would be involved. Viewing as much of the whole dispute as possible, including all relevant relationships among parties to a conflict, would allow us to explore key dimensions of behavior and isolate the major factors influencing choice. Focusing on the explanation of specific dispute decisions would let us build on prior, partial pictures of decision making, but

incorporate enough additional variables to ensure that our models were sufficient to account for the expanded range of behavior we expected to observe through our surveys.

V. DEVELOPING THE DISPUTES-FOCUSED APPROACH: THE SEARCH FOR A UNIFIED EXPLANATORY SCHEME

The disputes-focused approach, as I have noted, is more of an orientation than a research strategy. The first phase of CLRP was committed to translating this approach into a research design. The approach chosen dictated a number of methodological decisions which are described in greater detail in subsequent papers. As Kritzer (1981b) explains, these included defining the "dispute" and selecting a sample that would incorporate disputes in courts and other third-party institutions, as well as disputes dealt with on a purely "bilateral" basis. We sought to define the dispute broadly enough to include lawsuits and similar controversies in other settings, yet narrowly enough to allow comparison of disputes observed in various settings. The sampling strategy was designed to capture representative disputes both from state and federal courts and from a series of "alternative" third-party institutions handling cases similar to those in the courts. We devised techniques to locate what we called "bilateral disputes," those which never reached any type of third party. We tried to construct surveys of lawyers and disputants that would yield comparable information on disputes from all these settings, and would also probe relationships between the parties and uncover factors influencing their dispute decisions as completely as possible.

Several of the papers in Part Two reflect CLRP's effort to develop a more integrated theory of dispute decisions. We observed that any dispute carries a wide range of potential dispute trajectories. Three major choice areas can be identified. The first is the field explored conceptually by Felstiner, Abel, and Sarat and empirically by Miller and Sarat: the series of decisions that determine the emergence of disputes. The second is the choice between bilateral negotiations and resort to some third party. Finally, there are the choices made within a given institution, such as the decision to settle a lawsuit or seek a trial. The CLRP surveys were designed to observe a wide variety of such choices in an effort to identify factors that might explain these decisions.

Our dilemma was the existence of many possible explanatory schemes: cost-benefit factors were stressed by economists (Posner, 1977); variations in subjective perception were identified by psychologists (Nisbett and Ross, 1980); differences in socioeconomic status, party relations, and institutional constraints were cited by sociologists of law and political scientists (e.g., Galanter, 1974); audiences, discourses, and other factors were noted by anthropologists. Could we develop a more interdisciplinary approach, a kind of social scientific "unified field theory" of the dispute? To explore the options, theoretical and conceptual papers were prepared. Each paper focused on some aspect of the dispute decision and explored the implications of various explanatory schema. Thus, Johnson looked at the role of the lawyer in shaping decisions made once disputes enter a lawyer's office, and stressed the role of economic incentives in lawyer decision making. An unpublished paper by Komesar (1979) and the Gollop-Marquardt (1981) study applied an economic mode of analysis to disputant decisions. At the same time, Coates and Penrod explored a variety of social psychological theories bearing on disputant choice. Miller and Sarat developed a survey of the early stages of disputes and a theoretical scheme, drawing heavily on social structural variables, to explain decisions made in these stages.

Because of time constraints imposed upon the project, we proceeded with survey design and instrument development at the same time that we undertook the theoretical work illustrated by the papers included here. For the purposes of instrument development, therefore, we employed a tentative and eclectic model of dispute decisions which drew on our own partially completed conceptual work and other ideas in the literature. Although this working theory was not drawn directly or exclusively from any of the papers printed here, it oriented our search for an explanatory focus and drew on insights provided by these authors. This way of looking at the dispute decision will help the reader understand why many of the papers which follow were commissioned.

Our working theory can be described as a "modified stakes" model of dispute decision making. We began by focusing on the decisions made by disputants. To explain these choices, we took the economic model, illustrated by Johnson and Gollop-Marquardt, as a starting point and assumed that a major determinant of decision making in a case would be the relationship between what the parties perceived to be at stake

and their estimates of the costs of various dispute choices (Posner, 1977). Parties would invest in litigation and other forms of dispute processing as long as the expected gain (or loss reduction) exceeded the cost. They would prefer the choice that offered the highest ratio of expected return to estimated expenditure. To the extent that stakes and costs are monetary (or can be expressed in monetary terms), this approach is quite straightforward. Incorporating nonmonetary goals and costs, however, presented significant difficulties.

We took the "stakes" model from the economists, recognizing nevertheless that it would not fully predict disputant decisions. What was needed was an analysis that included not only costs and stakes but also a series of other variables likely to influence dispute decision making. To do this, we added to the simple cost-benefit model a series of factors which could cause disputants to deviate from the dispute trajectories predicted by the economic model. These included such variables as (a) the existence and nature of past and expected future relationships between the parties; (b) "party capability"—i.e. personal and psychological characteristics of individual disputants and variation in the size and structure of organizational parties; (c) the type of lawyer used and the nature of fee arrangements and lawyer-client relations; and (d) a series of factors related to the type of dispute itself, including areas of law, legal complexity, forum, etc. As Kritzer (1981b) has indicated, the questionnaires sought information on all these variables.

This working theory served merely as a heuristic to orient data collection and analysis. It helped us pull together insights from a number of fields, including many ideas developed in the papers published here, and incorporate these ideas in our data collection effort. Further, the "modified stakes" model has served as the starting point for multivariate data analysis. However, this has truly been a "working theory," and certainly not a fully specified empirical model. It has changed over time as we developed our instruments and began analysis. It will continue to change as we complete the analysis task.

VI. LEARNING ON THE JOB

Our primary purpose in publishing these papers is to share our experience to date with the research community, realizing that neither we nor the rest of the scholarly world can fully evaluate CLRP at this time. Assessing what we have learned so far, three things should be underscored. The first is that we

have certainly not transcended disciplinary limits. I think, however, that we have improved on the situation as we found it. These papers represent progress in the application of specific disciplines to litigation phenomena. In comparison with much prior work, they are more detailed and realistic. Compare, for example, the papers by Johnson and Gollop-Marquardt to much of the literature on the economics of litigation. These papers add elements usually omitted or assumed away in economic models, such as the existence of potential conflicts between the economic incentives of lawyers and their clients, the problem of uncertainty, and the availability of the dispute prevention alternative. Similarly, Coates and Penrod have not merely set forth theories to explain variations in subjective perception of dispute experiences; they have related them to the dynamic "naming, blaming, claiming" progression articulated by Felstiner, Abel, and Sarat, which itself incorporates insights from psychological research. These two papers, read together, suggest how we may be able to use psychological knowledge better to understand actual dispute situations. Further, I think it is fair to say that these papers are sensitive to the limits of any *one* explanatory scheme; they are "other-discipline directed." To one degree or another, all recognize the limits of any one field to explain the dispute decision fully, and most are explicitly receptive to insights from other fields. Johnson's careful delineation of the nonmonetary factors affecting attorney decision making, which opens up the whole sociology and psychology of the profession, illustrates this tendency, as does Felstiner, Abel, and Sarat's effort to draw on a variety of disciplinary bases to develop a dynamic concept of dispute transformation.

While these papers seem to have benefited from our efforts to break down disciplinary barriers, they are far from the ideal of integrated theory. How, for example, could one easily incorporate the objective, pecuniary factors of concern to Gollop and Marquardt with those subjective perceptions explored by Coates and Penrod? For all our efforts, most of these papers remain grounded in their particular disciplines. Perhaps other-discipline orientation is all that can be hoped for, given the current structure of the social sciences.

A second feature of the CLRP papers is that they offer micro-level explanations of dispute decision making. They identify a wide range of factors that should influence these decisions, and our data, when fully analyzed, should tell us

much about the importance (or unimportance) of some of these factors. Necessarily, as both Kidder (1981) and Lempert (1981) have recognized, our approach led us to focus on the individual dispute and concrete dispute decisions rather than the macro-social role of courts in society.

Finally, we have learned a great deal about both the potential and the limits of surveys for studying civil cases in particular and disputes in general. Survey methods impose distinct limits on what can be learned about the things parties did and did not do, and their motives. In his comprehensive review of our methodological experience, Kritzer (1981b) identifies many of the problems we encountered. We learned, for example, that it is next to impossible to get information from all parties in any given dispute or lawsuit using telephone survey techniques. We found that many questions which were important from a theoretical standpoint could not be asked retrospectively. Finally, we learned that the survey method requires an operational definition of the dispute which does not catch, in its full sublety and complexity, the rich experience of many conflicts as they evolve over time and are transformed by various actors, experiences, and events.

VII. CONCLUSION

The papers in Part Two represent an important part of the work of CLRP in its early stages. They help clarify what must be explored if we are to apply the disputes focus to the study of civil litigation, provide information on the methods that can be used to do this, and report some of our earliest empirical findings. They carry forward a tradition and help identify some of the problems and possibilities of social science research on civil litigation and dispute processing. Limited, partial, and tentative as they are, they represent a contribution to a growing literature.

For references cited in this article, see p. 883.